the highly gifted. The question of flatness of the world had, with the masses, hardly an existence; no molecules of the brain were exercised by it; the disturbance occurred only among the learned. Is it for this reason that we find so few survivals, to-day, of those who believe the world is flat?

EDWARD S. MORSE.

SALEM, May 17, '98.

'THE NEW PSYCHOLOGY.'

TO THE EDITOR OF SCIENCE: Professor Stanley's interesting letter is timely and valuable; it calls attention to a fundamental difference in standpoint between two schools of psychologists. This difference has been indicated by Professor Cattell in the following statement: "As a science advances beyond the stage of crude observation it tends to become either quantitative or genetic." The former tendency has produced experimental psychology; the latter genetic psychology.

The standpoint of experimental psychology—as far as I can understand the principles of its representatives—can be briefly stated as follows: *Given a group of phenomena, called 'phenomena of consciousness;' required a determination of the laws according to which these phenomena are connected.* This is a problem similar to that of astronomy, physics, meteorology, geology, biology, political economy—in fact, of all the sciences. In the early stages of a science the only solutions possible are those of 'yes' and 'no;' *e. g.*, does the memory of an object improve with interest and the lapse of time? to which the answers are: 'yes' for the former and 'no' for the latter. The introduction of methods of measurement—which is the special achievement of the new psychology—renders quite a different solution possible. The question just stated becomes: *how* does the memory of an object depend on interest and the lapse of time? The answer is as follows: Denote all the possible factors that may influence the memory by a, b, $c, \ldots, i, \ldots, t, \ldots, x$. *Keeping all the circumstances except i constant,* determine the relation of dependence of the memory on i, which is simply a roundabout method of saying: Let $a, b, c, \ldots =$ const. and find $M=f(i)$, where M is the accuracy or uncertainty or some other property of memory in the particular case. The method of solution, familiar to all experimentalists (see p. 77 of 'New Psychology'), consists in varying i quantitatively and measuring the resulting variations in M; the results when properly treated give a formula connecting the two; this is known as a law of memory. The fundamental necessity for such work is the method of measuring the quantities considered.

Professor Stanley remarks: "We must first devise some method of measuring interest;" it follows that we cannot determine this law of memory because such a method has not been found. This is quite true; the proper reply is to devise such a method—an undertaking not difficult to any one trained in psychological experiments. We can, however, measure time, and have in a number of cases (Wolfe, Ebbinghaus) determined the laws of various kinds of memory as depending on time or $M=f(t)$. The ideal solution—which Professor Stanley seems to expect at the start—is $M=F(a, b, c, \ldots, i, \ldots, t, \ldots, x)$ or the determination of the complete law of memory as depending on every possible circumstance. Perhaps some day psychology will make some approximation to such a solution; at present it must remain content with determining single laws.

Professor Stanley is quite wrong in assuming that this method is peculiarly a physical method. It belongs no more to physics than to chemistry (see the late works on mathematical chemistry), to political economy (Carnot, Jevons, Fisher), to biology (Pearson). It is merely a fundamental method of thought which is applicable wherever measurements can be made. In fact, we can reply to Professor Stanley that his science of genetic psychology must inevitably come to the use of this very method. Every single factor influencing the life of an individual or a community acts to a degree depending on its intensity according to some law; supposing all other factors to remain constant, this law is given by its action under those circumstances. By carefully measuring the action of each factor and its result on each property of mental life, the genetic psychologist could state the result as a series of laws of mental development. To be sure, this is rather a difficult task to propose,

but we may confidently expect the beginnings of such a genetic psychology in the future. At any rate, in this field, as in most other fields, progress and profit are increased by greater exactness and care, by more accurate and convenient apparatus and by shorter and more definite methods. These elements are the ones which experimental psychology is trying to introduce into the exploration of mental life. The fact that these methods are somewhat new in psychological work gives us the right to call a system of them a 'new psychology.'

Professor Stanley's claim that biology is the main standpoint of psychology is quite justified —if 'psychology' means the science of mental development. It must be remembered, however, that there is a fundamental difference in aim and method which marks off experimental psychology from the other mental sciences. Its object is to determine the fundamental laws of mental activity in the adult human being under ordinary circumstances. The change of the problem to child-study, to the development of the individual or of the race, or to abnormal circumstances, produces closely related sciences. All these sciences are inter-dependent. In fact, all these sciences—as Professor Stanley implies —are needed for a concrete, practical understanding of mental life; nevertheless convenience and clearness sometimes require that attention should be concentrated on one of them at a time.

E. W. SCRIPTURE.

NEW HAVEN, CONN., May 20, 1898.

FOSSIL FULGUR PERVERSUM AT AVALON, N. J.

ON page 682 of SCIENCE the quotation from Captain Swain, of the Avalon Life Saving Station, N. J., with reference to the casting ashore of *Fulgur perversum* is slightly inaccurate. I now quote from his letter the passage I read at the Academy that "the conchs in question come ashore only during a strong northwest (not northeast) wind that happens immediately after a northeast or a southeast gale, a northwest wind is the only kind that will bring heavy substances ashore, it seems to make the surface current offshore, and this creates an under current on-shore." I have no doubt that *Fulgur perversum* at the locality is raked out of a fossil bed a short distance offshore, and that this off-shore wind after the on-shore gales favors the tides and currents in doing so.

LEWIS WOOLMAN.

THE DEFINITION OF SPECIES.

I HAVE stated in this JOURNAL (N. S., VI, 329) that I believe the quantitative study of variation to be the most pressing problem of biological science. I have consequently read with great interest the papers by Professor Davenport and Mr. Blankinship, on 'A Precise Criterion of Species' (page 685 above). It seems evident that for the definition of species we should not depend on a 'type specimen,' the one first found, in the best state of preservation or the like, but should collate a considerable number of specimens taken at random, and when the traits can be measured give the averages and the mean deviations. Then, as Mr. Davenport explains, we have double-humped curves showing a tendency for the type to split up, and these are of the greatest possible interest to the student of the causes of the evolution of species.

When, however, Mr. Davenport proposes to use a given relation between the height of the smaller hump and the depression between the humps*—namely 100 : 50—as a precise criterion

* This relation depends not only on the distance between the apices, but also on the relative number of specimens of the two types, which, of course, has nothing to do with the difference between the types. There are other cases in Mr. Davenport's paper where the statements seem scarcely to take account of the complexity of the problems. It is meaningless to say that 'in some cases fifty per cent. or even more of the individuals will occur at the mode' and that in this case the curve is steep. The number of individuals at the mode depends on the unit of measurement selected, and the steepness of the curve is arbitrary. The 'half range,' defined as three times the 'standard deviation' (error of mean square), is a theoretically impossible point, and could only be determined approximately from thousands of specimens. Thus in Mr. Davenport's Fig. 9 the 'half range' of the right-hand curve is tripled by a single specimen. In all these cases Mr. Davenport neglects the probable errors which when reckoned show that his distinctions between species and varieties have no validity whatever. The data of Fig. 9 can be expressed by a curve with a single apex.